commenting on my article in NATURE, vol. xxvi. p. 86, and pointing out some errors as to the estimated advantage derived by the mimicking butterflies. On referring to my article, I find that I have, by an oversight, misstated the mathematical solution of the problem as given by Dr. Fritz Müller and confirmed by Mr. Meldola, and have thus given rise to some confusion to persons who have not the original article in the *Proceedings of the Entomological Society* to refer to. Your readers will remember that the question at issue was the advantage gained by a distasteful, and therefore protected, species of butterfly, which resembled another distasteful species, owing to a certain number being annually destroyed by young insectivorous birds in gaining experience of their distastefulness. Dr. Müller says: "If both species are equally common, then both will derive the same benefit from their resemblance—each will save half the number of victims which it has to furnish to the inexperience of its foes. But if one species is commoner than the other, then the benefit is unequally divided, and the *proportional advantage* for each of the two species which arises from their resemblance is *as the square* of their relative numbers." This is undoubtedly correct, but in my article I stated it in other words, and incorrectly, thus: "If two species, both equally distasteful, resemble each other, then the number of individuals sacrificed is divided between them in the proportion of the square of their respective numbers; so that if one species (*a*) is twice as numerous as another (*b*), then (*b*) will lose only one-fourth as many individuals as it would do if it were quite unlike (*a*); and if it is only one-tenth as numerous, then it will benefit in the proportion of 100 to 1."

This statement is shown by Messrs. Blakiston and Alexander to be untrue; but as some of your readers may not quite see how, if so, Dr. Müller's statement can be correct, it will be well to give some illustrative cases. Using small and easy figures, let us first suppose one species to be twice as numerous as the other, *a* having 2000 and *b* 1000 individuals, while the number required to be sacrified to the birds is 30. Then, if *b* were unlike *a* it would lose 30 out of 1000, but when they become so like each other as to be mistaken, they would lose only 30 between them, *a* losing 20, and *b* 10. Thus *b* would be 20 better off than before, and *a* only 10 better off; but the 20 gained by *b* is a gain on 1000, equal to a gain of 40 on 2000, or four times as much *in proportion* as the gain of *a*. In another case let us suppose *c* to consist of 10,000 individuals, *d* of 1000 only, and the number required to be sacrificed in order to teach the young birds to be 110 for each species. Then, when both became alike, they would lose 110 between them, *c* losing 100, *d* only 10. Thus *c* will gain only 10 on its total of 10,000, while *d* will gain 100 on its total of 1000, equal to 1000 on 10,000, or 100 times as much *proportional gain* as *c*. Thus, while the gain in actual numbers is inversely proportional to the numbers of the two species, the *proportional* gain of each is inversely as the *square* of the two numbers.

I am, however, not quite sure that this way of estimating the *proportionate* gain has any bearing on the problem. When the numbers are very unequal, the species having the smaller number of individuals will presumably be less flourishing, and perhaps on the road to extinction. By coming to be mistaken for a flourishing species it will gain an amount of advantage which may long preserve it as a species; but the advantage will be measured solely by the fraction of *its own numbers* saved from destruction, not by the proportion this saving bears to that of the other species. I am inclined to think, therefore, that the benefit derived by a species resembling another more numerous in individuals is really in inverse proportion to their respective numbers, and that the proportion of the squares adduced by Dr. Müller, although it undoubtedly exists, has no bearing on the difficulty to be explained.

ALFRED R. WALLACE

MR. A. R. WALLACE has been so good as to forward me the extract from the *Japan Mail* above referred to, together with his reply. The article in question bears the title, "Protection by Mimicry—a Problem in Mathematical Zoology." The authors, while admitting the broad principles involved in Dr. Fritz Müller's theory, fail to see why the advantage derived by the mimicking species, in cases where the latter is less numerous than the model, should be as the square of the relative numbers. They admit that "the ingenious explanation seems perfectly satisfactory," but the proportional benefit appeared to them exaggerated. Mr. Wallace has now, I think, cleared up the misunderstanding with reference to this part of the question, but it may be of use in assisting towards the further discussion of the problem if I here give the simple algebraical treatment adopted in the original paper.

Let a_1 and a_2 be the numbers of two distasteful species of butterflies in some definite district during one summer, and let n be the number of individuals of a distinct species which are destroyed in the course of a summer before its distastefulness is generally known. If both species are totally dissimilar, then each loses n individuals. If, however, they are undistinguishably similar, then the first loses $\frac{a_1 n}{a_1 + a_2}$ and the second loses $\frac{a_2 n}{a_1 + a_2}$. The absolute gain by the resemblance is therefore for the first species, $n - \frac{a_1 n}{a_1 + a_2} = \frac{a_2 n}{a_1 + a_2}$; and in a similar manner for the second species, $\frac{a_1 n}{a_1 + a_2}$. This absolute gain, compared with the total numbers of the species, gives for the first (A_1), $\frac{a_2 n}{a_1(a_1 + a_2)}$, and for the second ($A_2$), $\frac{a_1 n}{a_2(a_1 + a_2)}$. We thus have the proportion, $A_1 : A_2 = a_2^2 : a_1^2$.

With reference to Mr. Wallace's concluding paragraph, I may point out that the advantage of the mimic is "measured solely by the fraction of *its own members* saved from destruction." Thus, taking his last example, the species *c* saves only 1/1000 of its whole number, and *d* saves 1/10 of its whole number by the resemblance to *c*. The fact that these numbers stand to one another in the ratio of $1 : 10^2$, whilst $c : d = 10 : 1$, is a mathematical necessity from which I do not see how we can escape. As the numerical disproportion between the species increases, the advantage derived by the more abundant insect is practically a vanishing quantity; whilst, on the other hand, if the two species are equal in numbers, it is obvious that they both derive the same advantage, each losing only half the number that it would if there was no resemblance between them.

It must not be forgotten in considering the question of mimicry between two nauseous species that the foregoing calculations apply only to the case where the resemblance is perfect, *i.e.* so exact that the insects are absolutely undistinguishable by their foes. The initial steps may be hastened in these cases by the near blood-relationship of the species, and it is a remarkable circumstance that large numbers of species belonging to different distasteful genera have a close similarity of wing-pattern, although the distinctness of the genera has never been called in question. But the genera concerned, although distinct, are very closely related, and this is quite in accordance with the views here advocated.

The general question as to the persecution of distasteful butterflies by young inexperienced birds, &c., is certainly one on which much work remains to be done, and very great service could be rendered if naturalists residing in the tropics would undertake some systematic experiments in this direction. My friend, Mr. W. L. Distant, the author of the "Rhopalocera Malayana," has already given reasons in these columns (vol. xxvi. p. 105) for disbelieving in any such want of experience, and I have discussed this phase of the question with him elsewhere (*Ann. and Mag. Nat. Hist.*, December, 1882).

R. MELDOLA

On the Value of the "Neoarctic" as One of the Primary Zoological Regions

IN the *Proceedings of the Academy of Natural Sciences of Philadelphia* (December, 1882) Prof. Angelo Heilprin has an article under the above title, in which he seeks to show that the Neoarctic and Palæarctic should form one region, for which he proposes the somewhat awkward name "Triarctic Region," or the region of the three northern continents. The reasons for this proposal are, that in the chief vertebrate classes the proportion of peculiar forms is less in both the Neoarctic and Palæarctic than in any of the other regions; while, if these two regions are combined, they will, together, have an amount of peculiarity greater than some of the tropical regions.

This may be quite true without leading to the conclusion argued for. The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations—peculiarity due to the absence of widespread groups, and geographical individuality. The absence of the

families of hedgehogs, swine, and dormice, and of the genera *Meles, Equus, Bos, Gazella, Mus, Cricetus, Meriones, Dipus*, and *Hystrix*, among mammals; and of the important families of flycatchers and starlings, the extreme rarity of larks, the scarcity of warblers, and the absence of such widespread genera as *Acrocephalus, Hypolais, Ruticilla, Saxicola, Accentor, Garrulus, Fringilla, Emberiza, Motacilla, Yunx, Cuculus, Caprimulgus, Perdix, Coturnix*, and all the true pheasants, among birds, many of which are groups which may almost be said to characterise the Old World as compared with the New, must surely be allowed to have great weight in determining this question.

The geographical individuality of the two regions is of no less importance, and if we once quit these well-marked and most natural primary divisions we shall, I believe, open up questions as regards the remaining regions which it will not be easy to set at rest. There runs through Prof. Heilprin's paper a tacit assumption that there should be an equivalence, if not an absolute equality, in the zoological characteristics and peculiarities of all the regions. But even after these two are united, there will remain discrepancies of almost equal amount among the rest, since in some groups the Neotropical, in others the Australian, far exceed all other regions in their speciality. The temperate and cold parts of the globe are necessarily less marked by highly peculiar groups than the tropical areas, because they have been recently subjected to great extremes of climate, and have thus not been able to preserve so many ancient and specialised forms as the more uniformly warm areas. But, taking this fact into account, it seems to me that the individuality of the Neoarctic and Palæarctic regions is very well marked, and much greater than could have been anticipated; and I do not think that naturalists in general will be induced to give them up by any such arguments as are here brought forward.

ALFRED R. WALLACE

A Remarkable Phenomenon.—Natural Snowballs

I TAKE the liberty of inclosing a copy of an account of natural snowballs which I furnished to the *Courant* newspaper in this place. It may be well to state that the distance from Long Island Sound to Massachusetts is some seventy miles, and that the Connecticut Valley Railroad is about fifty miles long, and runs close to the bank of the Connecticut River for some forty miles; the rolls of snow on the frozen river are said to have been very large and handsome. SAMUEL HART

Trinity College, Hartford, Conn., U.S.A., February 22

On Tuesday evening a light but damp snow fell upon the crust that had formed over the snow of Sunday's storm; and the south wind, which arose at a later hour, produced an unusual phenomenon. Wednesday morning the college campus, the park, and vacant lots everywhere hereabouts were seen to be strewn with natural snowballs, some of them resembling spheres with diameters of from one to nine inches or more, and others looking very much like rolls of light cotton batting, having a cylindrical shape, but in nearly every case with a conical depression at each end reaching nearly or quite to the middle. It was easy to see how the balls had been formed, as it is easy to see how boys roll up the snow for their forts. The wind had in each case started a small pellet of the moist snow, and it had rolled along until it grew so large that the wind could move it no further. The ball not only increased in diameter as it rolled, but also grew gradually in length as a little more of the snow stuck to it on each side, and thus the snow was formed into the peculiar shape described—that of a cylinder with a hollow at each end, as if a long isosceles triangle were rolled up, beginning at its vertex. The largest of the cylinders measured on the college campus had a diameter of twelve inches and a length of eighteen inches, while others in the fields in the neighbourhood seemed much larger. The path of the balls could in many cases be readily traced for a distance of twenty-five or thirty feet. The snow, it should be added, was not at all closely packed, but lay together very lightly and yielded to a slight touch, so that it was impossible to move a ball without breaking it.

Observers in other parts of the city report that some balls were seen of the size of a barrel which left tracks behind them for more than sixty feet. From East Hartford it is reported that they studded the fields thickly, especially in places where the wind had a long range, and were of every size to that of a half bushel or larger. Similar balls were seen yesterday morning in many places from the Sound north to Massachusetts. All along the line of the Valley Railroad they appeared on every rod of ground, and at some places they had left tracks showing that the wind had blown them in every direction, even in some cases up hill.

This interesting phenomenon, though quite unusual, has been noticed before in different places in this country and elsewhere, the most striking instance on record being one which was observed in New Jersey in 1808; this was in the daytime, when the whole process could be watched. On this occasion some of the masses of snow which were rolled up by the wind attained a diameter of three feet. They appear to have been seen, however, over an area of only some four hundred acres, whereas the snowballs yesterday were spread thickly over many square miles.

[We have received a communication on the same subject from Prof. Brocklesby of Hartford.—ED.]

The Late Transit of Venus

I AM told that, in referring to the observations on the late transit of Venus which were made from a station on our college grounds by the astronomers of the German Imperial Commission, you speak of them as using the photographic process. This is not correct; besides contact observations they restricted themselves to the use of the heliometer. The first and the second contacts were not seen by reason of clouds; but four half sets and six full sets of heliometric measurements were made—128 in all. The third and the fourth contacts were observed by the German astronomers and by myself.

SAMUEL HART

Trinity College, Hartford, Conn., U.S.A., February 22

Rankine's "Rules and Tables"

I DO not know upon what authority your reviewer of Rankine's "Rules and Tables" bases his dictum that the r in the rule for the extension or compression of a spiral spring should be to the second power instead of to the third power. Prof. Rankine's view was that it should be r^3. I would refer your reviewer to vol. xviii. of the *Transactions of the Institution of Engineers and Shipbuilders in Scotland*, where he will find, amongst other results of an experimental committee's investigations upon the important question of the loading of safety-valves by such springs, that the *third* power of the radius or diameter of the spring is also used. W. J. MILLAR

Glasgow, March 10

[The formula given by Mr. Millar is, the writer of the notice informs us, perfectly correct, and the error is his.—ED.]

Meteors

ABOUT five minutes past seven this evening I saw the most beautiful "*shooting star*" I have ever witnessed. It was moving from east to west directly over this town, and disappeared at an apparent distance of ten or twelve miles, after traversing an arc of about 75° as I saw it. It was visible whilst one might count ten or twelve at the usual rate of speaking. In its course it not only left a most unusually long train of light behind, but whole pieces kept *dropping*. What appeared is thus best described. These pieces followed the original for a space, leaving perceptible lines of light. Probably ten or a dozen such pieces were broken off during the time I was looking. Some idea of it may be gathered from the fact that for a time I thought it was a rocket. The light was remarkably white, the brilliance much above that of Venus at any time, and its rate of motion slow. The most remarkable feature, however, was the continuous breaking away of pieces, which left in turn visible trains of light. THOMAS MASHEDER

The Grammar School, Ashby-de-la-Zouch, March 17

IN NATURE, vol. xxvii. p. 434, reading somewhat hastily, I took the brilliant meteor there mentioned to be one I myself saw. Reading more carefully, however, in last week's issue, I see that both day and hour and direction differ. On March 4, about 8.45 p.m., a very large and bright meteor passed at a low altitude from south to north. It was of a greater apparent size than Venus, quite as bright, but with a greener light. The motion was slow, no train; it only became incandescent during